

HOW MUCH DO DECOUPLED PAYMENTS AFFECT PRODUCTION? AN INSTRUMENTAL VARIABLE APPROACH WITH PANEL DATA

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How much decoupled payments, such as direct payments in the U.S., affect agricultural production remains an open empirical question with implications for policy. Using data from multiple years of the Census of Agriculture, we exploit a provision of the 2002 Farm Act that departed from previous policy by making oilseeds eligible for direct payments, thus increasing payments to areas that historically produced more oilseeds. Our instrumental variable estimates, in contrast to OLS estimates, suggest that changes in payments over the period 2002 to 2007 had little effect on aggregate production at the ZIP-code level.

Key words: decoupled payments, government payments, program crops, supply response, trade policy.

Introduction

Agricultural support payments that cause no or minimal production distortions can be categorized as “green-box” payments and are therefore exempt from World Trade Organization (WTO) restrictions. In the United States, production flexibility contract payments, according to the 1996 Federal Agricultural Improvement Reform Act (1996 Farm Act), and direct payments, according to the 2002 Farm Security and Rural Investment Act (2002 Farm Act), are considered “decoupled” because they are based on historical yields and acreage, not on current acreage, production, or prices. The extent to which such decoupled payments stimulate production and distort trade has emerged as a point of dispute in World Trade Organization negotiations (FAO 2005; Sumner 2005; USTR 2004). In 2007, Canada used the WTO Dispute Settlement process to Consult the United States, charging that U.S. corn subsidies suppressed corn prices in Canadian markets from 1996 to 2006 (Schnepf and Womach 2008). The charges were later postponed pending the Doha negotiations (Schnepf 2010).

Estimating how much payments affect aggregate supply is challenging because areas that had more acreage and higher yields of program crops in the past will receive more payments compared to less agriculturally productive areas. We would observe a spurious correlation between payments and yields, for example, if areas with higher yields in the past, and therefore greater government payments, experienced greater subsequent yield growth. We address the possibility of a spurious correlation between decoupled government payments and production by exploiting a provision of the 2002 Farm Act that allowed farmers to update their acres eligible for payments (their “base acreage”) to include soybeans and other oilseeds, which were historically ineligible for decoupled payments. Areas growing more oilseeds from 1998 to 2002, therefore, experienced an increase in government payments after 2002 relative to areas with less historic oilseed acreage. Using historic oilseed production as an instrument for future changes in payments, we estimate how an increase in payments affects growth in the value of program crop production (the supply response) and total cropland harvested (the response on the extensive margin) at the ZIP-code level¹.

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¹ ZIP codes refer to geographic units and are used by the U.S. postal service to deliver mail. They are typically much smaller than a U.S. county. To view the geographic distribution of ZIP codes, see <http://maps.huge.info/zip.htm>.

We estimate the response to payments using a sample of ZIP codes that accounts for 81% of the national production of program crops². To our knowledge, this is the first article to use an exogenous policy change and panel data to empirically estimate the supply response to decoupled payments.

There are four main ways that decoupled payments could affect production: risk, credit constraints, labor participation, and expectations (see OECD 2005 and Bhaskar and Beghin 2009). Hennessy (1998) articulated the risk mechanism and showed that decoupled payments could stimulate production by reducing farmers' absolute level of risk aversion (a wealth effect) by decreasing absolute risk aversion or, if payments are linked to shocks (e.g. price floors), by decreasing the variability of farm profits (an insurance effect). Several studies have incorporated the link between decoupled payments and risk in simulation models to estimate how payments affect supply (Young and Westcott 2000; Anton and Le Mouel 2004; Sckokai and Moro 2006). Serra et al. (2006) extended the approach to allow for input and output price risk. Using data from Kansas farms, Serra, Goodwin, and Featherstone (2011) find empirical evidence of decreasing absolute risk aversion, but also that the elasticity between decoupled payments and output is very small (0.00043). This is consistent with Just (2011), who finds that decoupled payments would have to increase operator wealth substantially to have a large effect on production through the risk aversion mechanism alone.

Even if the effect of payments on risk aversion is negligible, under imperfect credit markets, payments could increase output by providing farmers with financial capital for short-term liquidity needs or longer-term investment. Goodwin and Mishra (2006) find little evidence of an interaction effect between payments and farm debt-to-asset ratios when determining acreage. However, it is hard to interpret the results since payments affect asset values and potentially debt as well. By increasing land values and collateral for loans, payments may ease credit constraints and in turn permit greater investment and output (Roe, Somwaru, and Diao 2003). Roberts, Kirwan, and Hopkins (2003); Goodwin, Mishra, and Ortalo-Magne (2003); and Kirwan (2009) econometrically estimated the extent that

decoupled payments are capitalized into land values. Model-based empirical research has also considered how payment-induced increases in land values affect agricultural production (Dewbre, Anton, and Thompson 2001; Gohin 2006).

A third way that payments could affect production is by changing how farm households allocate their labor. Ahearn, El-Osta, and Dewbre (2006) and El-Osta, Mishra, and Ahearn (2004) econometrically estimated the effect of decoupled payments, which raise household wealth and thereby influence labor-leisure tradeoffs on household labor supply both on and off the farm. The above studies find that farm operators who receive more payments tend to supply less labor off the farm and work more hours on the farm, a finding that is consistent with Key and Roberts (2009), who show how farmers with preferences for farm (versus off-farm) work could respond to higher decoupled payments by working more on the farm and less elsewhere.

Even when current payments are decoupled from production, farmers may alter their production decisions to maximize future payments from expected, though uncertain, policy changes (Lagerkvist 2005; Sumner 2003). The 2002 Farm Act, which extended the fixed decoupled payments of the 1996 Act, gave producers an opportunity to update their base acreage and yields, and allowed them to include acreage in common oilseeds like soybeans and rapeseed in their base. Prior to 2002, farmers may have altered planting decisions in anticipation of the base updating, even though current payments were decoupled from current production (Coble, Miller, and Hudson 2008).

While understanding the mechanisms through which payments may affect production can inform policy decisions and improve modeling efforts, the marginal effect of payments on output has clear implications for trade policy. Using survey data to econometrically estimate the effect of decoupled payments on agricultural production, however, presents challenges. Decoupled payments originate from agricultural programs open to all program crop farmers, and program participation is often nearly universal. This makes it difficult or impossible to distinguish between a treatment and control group – a prerequisite for standard program evaluation. In most instances it is also unclear what causes a variation in payments across observationally similar farms, opening the possibility that unobservable factors could be associated

² We define program crops to be oilseeds, corn, oats, barley, sorghum, and wheat – that is, crops whose production regions overlap substantially.

with both program participation (or payment levels) and production. Further complicating the identification of the effect of payments, changes in agricultural subsidy policies generally occur simultaneously across the nation and make it difficult to distinguish the effects of a policy change from time-varying factors like prices and technology. Consequently, econometric analyses should address concerns about omitted variables correlated with both agricultural supply and government payments. Furthermore, the most detailed source of data on U.S. farms, the Agricultural Resource Management Survey (ARMS), is applied to a different sample of farms each year, thus precluding panel data approaches that aid in separating the effect of payments from confounding factors.

Goodwin and Mishra (2006) contribute to the econometric literature by estimating the relationship between coupled and decoupled payments with acreage using multiple years from the cross-sectional ARMS survey. These authors estimate a linear relationship between county-level payments per acre and farm-level crop acreage conditioning on contemporaneous farm and county variables. The authors recognize the standard critique of ignoring unobservable variables correlated with the outcome of interest. The greater concern, however, is the endogeneity of payments (total or per acre) caused by the mechanical relationship between yields and acreage and payments. Counties with more acreage or higher yields will have greater payments. We would expect a correlation between county-level payments and farm-level acreage since what is true of the county is likely true of farms within the county.

O'Donoghue and Whitaker (2010) address payment endogeneity by examining changes in payments caused by the 2002 Farm Act and changes in acres harvested. The authors use multiple years from the ARMS to construct cohorts of farms from the same state and commodity group to create a pseudo panel. Their treatment of the endogeneity of payments contributes to the literature, but the restrictive assumptions of the pseudo-panel and the small sample size (64 cohorts) leaves room for improvement. The ARMS sample is not designed to be representative at most state-commodity cohorts, meaning that cohort average values calculated from the sample may poorly reflect the true population value for the state-commodity group. Cohort average values may also vary substantially from year to year due to changes in the sample design – the

ARMS targets different commodities in different years. Moreover, the study examines the effect of payments on cropland harvested, not on production. Although acres harvested and production are related, increased yields or changes in crop mix could change the value of production, but not that of cropland harvested.

Gardner, Hardie, and Parks (2010) adopt a county-level approach to estimate the relationship between payments and land use in 1987, 1992, and 1997, and find that decreasing program payments by half from their observed levels would have decreased cropland acreage by 89 million acres (22%). Instrumental validity and measurement error in land use data notwithstanding, the result suggests a large effect of payments on production. This link is unsurprising since commodity policy in 1987 and 1992 explicitly linked payments to production decisions. It remains to be seen whether the decoupling that accompanied the 1996 Farm Act and maintained in the subsequent 2002 Farm Act had a similar effect.

Arguably, the most substantial change to the direct payment program in 2002 was the inclusion of oilseeds³ as a program crop. While oilseed producers received no production flexibility contract payments (the predecessor to direct payments) in 2001, the 2002 Farm Act allocated roughly \$600 million for oilseed producers per crop year, starting with the 2002 crop year (Farm Service Agency 2011). Direct payment rates specified in 2002 for other crops were similar to the rates applied to production flexibility contracts in 1997 under the 1996 Farm Act. Under the 2002 policy, payments for oilseeds were based on plantings and yields from 1998 to 2002.⁴ Hence, the 2002 Farm Act increased payments for some farmers after 2002, and the increase was exogenous to their post-2002 planting decisions. This exogenous variation in decoupled payments allows us to identify the effect of payments on the value of production and acres of cropland harvested, and makes it credible to assert that the estimated association is causal.

³ The oilseeds included soybeans, sunflower seed, canola, rapeseed, safflower, mustard seed, flaxseed, crambe, and sesame.

⁴ In practice, there were several options for updating base acreage under the 2002 Farm Act. The most common option allowed a soybean base to be added to base acres, where the soybean base was the minimum of 1) the average of program crop acreage 1998–2001 minus production flexibility contract base acres from the 1997 Farm Act and 2) average soybean acreage 1998 to 2002. For more details, visit: http://www.farmdoc.illinois.edu/manage/newsletters/fefo02_16/fefo02_16.html.

We offer several contributions to the empirical literature on decoupled payments. First, the exhaustive nature of the Census data allows us to aggregate farm-level observations to the ZIP-code level, which permits us to estimate the aggregate supply response across the nation using a large number of geographic units. Second, by examining the value of program crop production and total cropland harvested, we can identify the total supply response for program crops and the acreage response for crops in general, which is an improvement on existing studies that only examine the acreage effect and often only acres in program crops. Third, constructing a panel from multiple Census years allows us to control for growth trends correlated with payments and production that could bias estimates. Finally, and perhaps most importantly, the policy change in 2002 that changed payments to farmers based on past planting history provides clear guidance for selecting an instrument for the change in decoupled government payments. The use of an instrumental variable reduces the possibility that temporally-correlated unobservable variables bias our results.

Empirical Model

It is often assumed that farm operators make production decisions to maximize their expected utility. As noted in our discussion of the literature, attitudes towards risk, expectations about future policy changes, or preferences for farm versus off-farm work could all enter into an agent's optimization problem. In addition, imperfections in credit, labor, land or other markets could constrain production decisions. The complexity of the optimization problem precludes deriving a feasible structural production response equation. Instead, we posit a reduced-form equation describing the change in production across time that supposes observing a ZIP code in three distinct years: 2007, 2002, and 1997.

Let y_{i07} be the outcome for ZIP code i in 2007 (either the value of production of program crops or total cropland harvested). The outcome depends on its past values y_{i02} and y_{i97} , changes in government payments resulting from the 2002 Farm Act ($\Delta GP_{i,02-07}$), covariates X_{i02} and X_{i97} from the previous two periods, a region-specific effect $\mu_{r(i)}$, and an idiosyncratic shock ε_{i07} . Supposing that the relationship between y_{i07} and the other

variables can be approximated with a linear functional form, we have:

$$(1) \quad y_{i07} = \delta_0 + \delta_1 y_{i02} + \delta_2 y_{i97} + \delta_3 X_{i02} + \delta_4 X_{i97} + \theta \Delta GP_{i,02-07} + \mu_{r(i)} + \varepsilon_{i07}$$

where $\Delta GP_{i,02-07} = GP_{i07} - GP_{i02}$.

Including y_{i02} controls for the previous level of the dependent variable, and to the extent that payment flows affect aggregate outcomes, it also controls for payments received under the 1996 Farm Act. The second lag, y_{i97} , controls for different long-term growth trends across ZIP codes. Furthermore, including the 1997 values of the dependent variable and X variables makes the model robust to serial correlation in the error of the form AR(1) (Wooldridge 2009), which we show explicitly in the supplementary appendix.

An alternative approach would be to use a first difference (FD) model where the outcome is the change in y from 2002 to 2007. The FD model, however, does not control for different growth trends between high and low oilseed areas. Adding a lagged differenced term ($y_{i02} - y_{i97}$) is problematic because it is mechanically correlated with the differenced error term ($\varepsilon_{i07} - \varepsilon_{i02}$). Moreover, it does not allow for a non-linear growth trend because the lagged differenced term requires an increase in y_{i02} to have the same effect as a decrease in y_{i97} , since they have the same effect on the change from 2002 to 2007. A model with lagged dependent variables, in contrast, allows the coefficients on each lag to be different and to even have different signs, which could occur if strong crop rotation effects were at play.⁵

The 1996 Farm Act determined annual payment flows from the time of its enactment until the 2001 crop year, with payments for the 2001 crop year being sent in the 2002 calendar year and reported in the 2002 Census of Agriculture. The 2002 Farm Act determined payments for the crop years 2002 to 2007, and because it set constant payment rates for the entire period, payments reported in the 2007 Census reflect the annual flow of payments under the 2002 policy.⁶ The change in payments from 2002 to

⁵ It is common for farms to rotate among crops of different value and, in some areas, to rotate land in and out of production. Variation in rotations across space could introduce temporal correlations that affect estimates.

⁶ The change from the PFC payments to direct payments in the 2002 Farm Act can be seen at <http://www.ers.usda.gov/Briefing/FarmPolicy/gallery/govpay/slide4.gif>. The figure also shows that few direct payments arrived in 2002. The jump in payments in 2003 is

2007, therefore, reflects a discrete shift in payment flows caused by differences in the 1996 and 2002 Farm Acts.⁷

Assuming that $\Delta GP_{i,02-07}$ is uncorrelated with ε_{i07} and estimating (1) with OLS would likely be an improvement upon cross-sectional models since it uses variation in payments over time while controlling for past realizations of the variable of interest. Still, the exogeneity of $\Delta GP_{i,02-07}$ is a tenuous assumption given the mechanical relationship between production and payments. At the farm level, the endogeneity problem would be severe since renting or buying program acres for any reason would increase a farm's payments and output. At the aggregate level, the problem is less severe since once updating has occurred, base acres and yields are largely fixed in the aggregate; for the years governed by the 2002 Farm Act, payments in a ZIP code should have varied little from one year to the next. However, updating base acres and yields implies that areas with higher yield or acreage growth would receive a larger increase in payments compared to areas with less growth. Identifying the effect of payments is confounded if areas with higher yields and acreage in the past experience higher than average growth in the future – a very plausible scenario.

To identify the causal effect of payments on production, we instrument for the change in payments using the value of oilseed production averaged from 1997 and 2002. Because the policy change allowed farms to receive payments based on past oilseed acreage and yields, ZIP codes associated with greater oilseed production in previous periods would have experienced a greater increase in payments from the policy. A good instrument will be strongly correlated with the endogenous regressor, in this case $\Delta GP_{i,02-07}$, and uncorrelated with the error term, ε_{i07} . We show in a following section

that because of the policy change, past production of oilseeds is a strong predictor of future changes in payments. We also perform a diagnostic that casts light on the extent to which possible correlation with the error term may affect our results.

To aid in identification, we include in the vector of control variables (X) the acres of idle agricultural land (land in the Conservation Reserve Program, plus land in fallow or other idle states) and the total amount of tillable land (land out of production, plus harvested land and pasture), corn yields, the median farm size in the ZIP code measured by acres harvested, and a linear and quadratic term for the median age of farm operators in the ZIP code. Acres of tillable land and acres of idle agricultural land capture the land constraints in a ZIP code, while the corn yield reflects land quality. The median farm size captures the scale of the typical operation, which could be important given economies of scale and trends towards larger but fewer farms. The age terms control for changes in production linked to the life stage of the typical farm operator.

Including the region term $\mu_{r(i)}$ is important in a national analysis covering landscapes with different agro-climatic conditions and crop mixes. The region variable is based on a classification provided by the USDA/Economic Research Service that groups counties according to crop reporting districts and farm characteristics like crop mix.⁸ The region term is arguably preferable to a state-level term since states whose boundaries were created based on political considerations often encompass areas with very distinct types of agriculture.

We estimate two outcome equations – one relating the change in total government payments to the value of production of program crops (valued at 2002 prices) and another relating payments to cropland harvested:

$$(2) \quad VP_{i07} = \delta_0 + \delta_1 VP_{i02} + \delta_2 VP_{i97} + \delta_3 X_{i02} + \delta_4 X_{i97} + \theta \Delta GP_{i,02-07} + \mu_{r(i)} + \varepsilon_{i07}$$

$$(3) \quad CH_{i07} = \delta_0 + \delta_1 CH_{i02} + \delta_2 CH_{i97} + \delta_3 X_{i02} + \delta_4 X_{i97} + \theta \Delta GP_{i,02-07} + \mu_{r(i)} + \varepsilon_{i07}$$

largely an artificial jump caused by the timing of the enactment of the 2002 Farm Act. Some of the payments for the 2002 crop year normally would have reached farmers late in the 2002 calendar year, but due to delays in implementing the 2002 Act, most of these payments came in the 2003 calendar year.

⁷ The other commodity-related provisions changed little between the 1996 and 2002 Farm Acts. Marketing loans and loan deficient payments existed under the 1996 Farm Act in similar form, while the 2002 Farm Act's counter-cyclical payment program replaced Marketing Loss Assistance payments from the amended 1996 Act (Plato et al. 2007). The continuity of price-related programs suggests that the risk borne by producers did not change substantially in 2002, and it ameliorates concerns that policy changes outside of expanding direct payments to oilseeds undermine our identification strategy. For a side-by-side comparison of specific provisions of the two farm acts, see <http://www.ers.usda.gov/Publications/AP/AP022/AP022.pdf#Title1>.

⁸ See www.ers.usda.gov/publications/aib760/aib-760.pdf for a map of the region and more details.

We instrument for ΔGP_{it} using the reduced form equation:

$$(4) \quad \Delta GP_{i,02-07} = \alpha_0 + \alpha_1 y_{i02} + \alpha_2 y_{i97} \\ + \alpha_3 X_{i02} + \alpha_4 X_{i97} \\ + \beta Oilseeds_{i,9702} \\ + \mu_{r(i)} + \nu_{i07}$$

where y is the lagged dependent variable – either the value of production or cropland harvested – depending on whether (2) and (3) is estimated, and $Oilseeds_{i,9702}$ is the average value of production of oilseeds over the two years.

Data

The ZIP-code values for each census year are calculated by aggregating farm-level data from the Census of Agriculture administered by the USDA National Agricultural Statistics Service (NASS). The census collects data on farm and operator characteristics every five years from most farms in the country, and response rates are generally high (more than 80%).⁹ To account for farms not on its mailing list or that do not respond, NASS assigns each farm a probability weight that reflects how many farms each respondent farm represents. We apply this weight when aggregating farms in each census year so that our variables reflect the population of farms in each ZIP code.

We define “program crops” as corn, soybeans, wheat, oats, barley, sorghum, canola, flaxseed, safflower, and sunflower. All of these crops were officially program crops after the 2002 Farm Act, but only corn, wheat, oats, barley, and sorghum were program crops in 1997. We focus on these program crops because their areas of production overlap substantially, leading to a more homogenous sample of ZIP codes in terms of weather and cropping patterns.

We calculate the value of program crop production for each Census year, holding prices constant at 2002 levels.¹⁰ Harvested cropland includes all acreage from which crops were

harvested, including forages; it captures the effect of payments on the true extensive margin because it counts each acre only once, even if two crops were harvested on the acre over the growing season. Furthermore, because the measure includes all land in crops, it is not affected by rotation or substitution among crops. Thus, while the value of production captures the total supply response of program crops, harvested cropland captures whether payments affect land in cultivation in general.

We define government payments as total payments received for participation in federal farm programs (excluding Commodity Credit Corporation loans or crop insurance payments), net of payments received for participation in the Conservation Reserve Program and the Wetlands Reserve Program. Because there is generally a one-year lag in the distribution of farm payments, payments reported for the 2002 calendar year in the Census of Agriculture correspond to the 2001 crop year, which was governed by the 1996 Farm Act. In 2002, decoupled payments comprised about 41% of federal payments net of conservation payments, while in 2007 they accounted for 70% (Farm Service Agency, 2011).¹¹ The smaller share of decoupled payments in 2002 reflects the substantial payouts from the Marketing Loan and Loan Deficiency Programs in response to low crop prices in 2001.

For the program crops considered, roughly three-quarters of the price-related payments in fiscal year 2002 went to oilseeds. The lack of a similar level of payments in fiscal year 2007 implies that price-related payments decreased from 2002 to 2007 more for oilseeds than for the other program crops. The estimated increase in government payments calculated from the 2002 and 2007 Census of Agriculture, therefore, likely underestimates the real increase in payments that historic oilseed-producing areas experienced relative to other areas. If decoupled payments affect farmer decisions, the observed response will be for an increase in payments larger than the observed increase in the data. Both OLS and IV estimates will then represent, if anything, an upper bound estimate of the supply response to decoupled payments.

An analysis of aggregate outcomes could be conducted at the ZIP code, county, or state

⁹ The census attempts to reach all agricultural operations that produce, or would normally produce and sell, \$1,000 or more of agricultural products per year. Data are primarily collected through the mail, with supplemental reporting on the Internet and non-response follow-ups by telephone and personal enumeration. The final response rate was 85.2% for the 2007 Census of Agriculture and 88.0% for the 2002 Census of Agriculture.

¹⁰ For all commodities except corn silage, prices come from the USDA NASS QuickStats webtool <http://www.nass.usda.gov/>

QuickStats/Create_Federal_All.jsp. For corn silage we use a price of \$20/ton.

¹¹ Outlays by crop, program, and year are available on the website of the Farm Service Agency. The percentages reported were calculated by the authors using FSA data.

Table 1. Descriptive Statistics of Key Variables for U.S. and Heartland Samples of ZIP Code

	U.S. Sample (N=6,573)			Heartland Sample (N=3,489)		
	Median	Mean	S.D.	Median	Mean	S.D.
Value of Production 2002 (\$1,000s) ^a	3,209	4,935	5,069	4,257	5,981	5,314
Change in Value of Production 2002–2007 (\$1,000s)	525	1,000	2,266	674	1,047	1,890
Cropland Harvested 2002 (Acres) ^b	19,287	27,022	24,404	20,417	26,838	21,052
Change in Cropland Harvested 2002–2007	–194	19	6,733	–275	–293	4,604
Payments 2002 (1,000s)	316	455	484	309	414	346
Change in Payments 2002–2007 (1,000s)	–40	–55	242	–20	–29	156
Idle Land 2002 (Acres)	1,831	4,542	10,990	1,647	3,151	4,302
Tillable Land 2002 (Acres)	26,339	36,797	36,085	26,038	33,468	25,586
Corn Yield 2002 (Bushels/Acre)	118	115	36	128	124	34
Farm Size 2002 (Acres)	96	172	210	127	177	165
Operator Age 2002	54	54	3	54	54	3

Source: U.S. Census of Agriculture, multiple years.

^aValue of Production includes only program crops and is calculated in each Census year using 2002 prices.

^bCropland Harvested includes all cropland harvested, including forages and non-program crops.

level. ZIP codes are used because they are the smallest geographic unit where farms can be located with the data, and thus provide the maximum number of observations and cross-sectional variation in the dependent and independent variables. Although a very small fraction of ZIP codes change over time, most changes have occurred in relatively urban areas with population growth and where agriculture is less prevalent, which mitigates this potential problem.

We only include ZIP codes that contributed a non-negligible amount to the national production of program crops. To trim the sample, we first calculate the value of program crops produced in the nation averaged across 1997, 2002, and 2007, then sort ZIP codes by their average value of production of program crops for these years, and finally calculate a cumulative sum for each ZIP code. Taking only the ZIP codes associated with 95% of the value of program crops leaves 8,467 ZIP codes. We also require each ZIP code to have a positive value for each covariate and some soybean production – the most common oilseed – in all census years. This leaves a total sample of 6,573 ZIP codes that together accounted for 81% of the total U.S. value of program crop production for the years 1997, 2002, and 2007.

To determine if our results are driven by regional heterogeneity, we also analyze a subset of ZIP codes located in the Heartland – a region that accounts for a large share of the national program crop production and, naturally, a large share of direct payments. Of the 6,573 ZIP codes used in the national analysis, 3,489 are located in

the Heartland – a relatively homogenous geographical region defined at the county level by the USDA/Economic Research Service. The Heartland region includes all counties in Illinois, Indiana, and Iowa, and some bordering counties in Kentucky, Minnesota, Missouri, Nebraska, Ohio, and South Dakota.

Table 1 presents descriptive statistics of key variables for the national sample of ZIP codes and for the subset located in the Heartland. Except for *Operator Age* and *Farm Size*, which are medians for the ZIP code, all variables are calculated by aggregating all farms in the same ZIP code. Monetary amounts are in 2002 dollars.¹² From 2002 to 2007, the average ZIP code saw the value of production increase by about 20%. At the same time, there was almost no change in the average acres of cropland harvested, and the median ZIP code even experienced a small decrease. The large increase in production without an increase in area suggests that farmers replaced non-program crops with program crops and/or increased the intensity of program crop cultivation through higher yielding varieties, greater input use, and possibly more double-cropping. The average ZIP code saw a 12% decrease in government payments in real terms relative to the 2002 level of about \$450,000. The descriptive statistics for ZIP codes in the Heartland follow similar patterns. The value of production increased by about 17% for the mean ZIP code, while

¹² To put monetary amounts in real terms, we use the “CPI research series using current methods, 1978–98,” provided by the Bureau of Labor Statistics: <http://www.bls.gov/cpi/cpirsdc.htm>.

Table 2. Oilseed Production and Changes in Payments

Variable	Entire U.S.		Heartland	
	Lags for VP	Lags for CH	Lags for VP	Lags for CH
<i>Oilseeds 1997, 2002</i>	0.143*** (0.013)	0.096*** (0.011)	0.224*** (0.032)	0.177*** (0.034)
<i>Value of Production 2002</i>	0.053 (0.041)		−0.037 (0.080)	
<i>Value of Production 1997</i>	−0.100** (0.049)		−0.062 (0.087)	
<i>Cropland Harvested 2002</i>		0.557*** (0.071)		0.792*** (0.064)
<i>Cropland Harvested 1997</i>		−0.318*** (0.090)		−0.806*** (0.080)
Controls for X_2002 and X_1997	yes	yes	yes	yes
Controls for Region	yes	yes	yes	yes
Observations	6,573	6,573	3,489	3,489
Adjusted R Squared	0.233	0.244	0.080	0.079
F-stat of zero coefficient on Oilseeds	124.4	71.8	48.4	26.8

Note: Asterisks (***, **, *) denote significance at the 1%, 5%, and 10% levels, respectively. Robust standard errors are in parenthesis.

area harvested decreased slightly. Payments also decreased in real terms.

Estimation and Instrumental Variable Diagnostics

To reduce the influence of outliers and be able to interpret coefficients as elasticities, we use the natural log to transform all the variables except *Operator Age*, *Corn Yield*, and *Farm Size*. The key explanatory variable of interest, the change in payments from 2002 to 2007, is defined as $\Delta GP_{i,02-07} = \ln(GP_{i07}) - \ln(GP_{i02})$.

Payments for oilseeds under the 2002 Farm Act were based on the acres and yields of oilseeds from 1998 to 2002. We use the value of oilseed production averaged using the years 1997 and 2002 ($Oilseeds_{i,9702}$) as our instrument for the change in payments caused by the 2002 policy change. When testing for the statistical relevance of an instrument, a rule of thumb is that the F-statistic for the null hypothesis, that the instrument coefficients are jointly equal to zero, should exceed 10 (Staiger and Stock 1997).¹³ Stock and Yogo (2005), however, find that weak instruments also distort the Wald test statistics based on IV estimates. To avoid such

distortions, their simulations imply that the F-statistic should exceed 16 in the case of one instrument and one endogenous regressor.

Estimating the first stage equation (4) reveals, as expected, that the value of oilseed production is strongly correlated with the change in total payments from 2002 to 2007 (Table 2). Using the national sample, the F-statistic for the oilseed production variable is 124 in the equation controlling for lagged value of production, and 71 in the equation controlling for lagged cropland harvested. The corresponding F-statistics for the Heartland are 48 and 26. We therefore dismiss concerns about weak instrument bias. The coefficients from the national sample suggest that a 1% increase in historic oilseed production is associated with a 0.09 to 0.14% greater growth in government payments from 2002 to 2007.

The IV models using the value of past oilseed production as an instrument for changes in payments are implemented using Two-Stage Least Squares. Robust standard errors allowing for heteroskedasticity are calculated.¹⁴ Results for the control variables in X are omitted from the tables; the full results are available in the supplementary appendix.

¹³ With instruments that are only weakly correlated with the variable they instrument for, the Two-Stage Least Squares estimator is biased towards the probability limit of the OLS estimator, with the bias occurring because of randomness in the first-stage fitted values (Bound, Jaeger, and Baker 1995; Angrist and Pischke 2009).

¹⁴ Angrist and Pischke (2009) show that if heteroskedasticity is modest, the finite sample bias of the traditional formula for homoskedastic standard errors is less than the bias of the robust sandwich estimator. The large sample size and the likelihood of significant heteroskedasticity given the range in ZIP code sizes support using the robust estimator. However, we do include a finite sample adjustment by multiplying the covariance matrix by $N/(N-K)$.

Table 3. Payments and Value of Production and Cropland Harvested: Entire U.S.

Variable	Value of Production		Cropland Harvested	
	OLS	IV	OLS	IV
<i>Change in Payments 2002-2007</i>	0.206*** (0.014)	-0.066 (0.069)	0.194*** (0.010)	-0.047 (0.057)
<i>Value of Production 2002</i>	0.627*** (0.031)	0.674*** (0.037)		
<i>Value of Production 1997</i>	0.255*** (0.037)	0.249*** (0.041)		
<i>Cropland Harvested 2002</i>			0.609*** (0.032)	0.793*** (0.062)
<i>Cropland Harvested 1997</i>			0.196*** (0.041)	0.123** (0.053)
IV different from OLS results? ^a		yes		yes
Controls for X_2002 and X_1997	yes	yes	yes	yes
Controls for region?	yes	yes	yes	yes
Observations	6,573	6,573	6,573	6,573
Adjusted R Squared	0.881	0.862	0.933	0.912

Note: Asterisks (***, **, *) denote significance at the 1%, 5%, and 10% levels, respectively. Robust standard errors are in parentheses.

^aThis refers to the result of a Durbin-Wu-Hausman test for endogeneity. The test was conducted at the 10% percent level.

Results

For outcome equations (2) and (3), the OLS estimates suggest a strong statistical relationship between payments and the value of production and cropland harvested. For the national sample (table 3), the coefficients imply that a 1% increase in payments is associated with a 0.206% increase in the value of production and a similar increase in cropland harvested. For the Heartland (table 4), the OLS estimates are more than one-third larger than the estimates from the national sample. In all cases, the OLS estimates are precisely estimated, with point estimates being 10 to 20 times larger than their standard errors.¹⁵

In contrast, the IV estimates at the national level (table 3) suggest that government payments had little effect on the value of program crop production or on the acres of cropland harvested, with the point estimates being negative and statistically indistinguishable from zero. A Durbin-Wu-Hausman test for the exogeneity of the change in payments rejects the null hypothesis of exogeneity at the 10% level, meaning that the OLS point estimates should be rejected based on endogeneity bias.¹⁶ For

the Heartland (table 4), the IV estimate is also substantially smaller than the OLS estimate for the value of production (0.012 compared to 0.293); for cropland harvested, the difference is smaller (0.139 compared to 0.272). The exogeneity of the change in payments is rejected in the first case but not in the second case (p value = 0.15).

A lack of precision in estimation may lead to a failure to reject the null hypothesis of no effect when the true effect is positive and possibly of economic importance. In the present case, however, at the 5% level we reject elasticities between payments and production and acres harvested larger than 0.072 and 0.067 for the national sample.¹⁷ For the Heartland, the upper bound on the confidence interval is

suspected endogenous variable on the excluded instrument and the control variables. The outcome variable is then regressed on the suspected endogenous variable, the control variables, and the residuals from the previous regression. If the coefficient on the residual is not statistically different from zero, then the hypothesis that the variable is exogenous cannot be rejected.

¹⁷ One concern is whether the results reflect production shocks in 2002 or 2007 that were correlated with the instrumented change in payments. A shock that decreased production in 2007 for areas that historically grew lots of oilseeds would reduce the estimate of the effect of payments on production. In 2007, national soybean yields were 11% lower than the average yield for 2002 to 2007, but much of the drop likely came from shifting higher quality land into corn, whose exceptionally high prices in 2007 caused corn acreage to reach a record level. Furthermore, historic oilseed-producing areas would have probably contributed substantially to the increase in corn acreage, which, because corn is a higher valued crop, would tend to bias our results for the value of production upwards. We also note that production shocks should affect cropland harvested less than the value of production. Both outcomes point to a small, if even present, effect of payments on production.

¹⁵ The small standard errors may suggest spatial correlation across ZIP codes. Including state dummy variables instead of regional variables is one way to capture a more localized spatial correlation. Doing so does not qualitatively change the results.

¹⁶ The Durbin-Wu-Hausman test can be used to test whether IV estimates are statistically different from the OLS estimates. The test is performed by first obtaining the residuals from regressing the

Table 4. Payments and Value of Production and Cropland Harvested: Heartland

Variable	Value of Production		Cropland Harvested	
	OLS	IV	OLS	IV
<i>Change in Payments 2002-2007</i>	0.293*** (0.021)	0.012 (0.113)	0.272*** (0.018)	0.139 (0.090)
<i>Value of Production 2002</i>	0.600*** (0.055)	0.638*** (0.066)		
<i>Value of Production 1997</i>	0.401*** (0.062)	0.398*** (0.072)		
<i>Cropland Harvested 2002</i>			0.682*** (0.055)	0.722*** (0.069)
<i>Cropland Harvested 1997</i>			0.140** (0.067)	0.132* (0.073)
IV different from OLS results? ^a		yes		no
Controls for X_2002 and X_1997	yes	yes	yes	yes
Controls for region?	yes	yes	yes	yes
Observations	3,489	3,489	3,489	3,489
Adjusted R Squared	0.915	0.898	0.934	0.929

Note: Asterisks (***, **, *) denote significance at the 1%, 5%, and 10% levels, respectively. Robust standard errors are in parentheses.

^aThis refers to the result of a Durbin-Wu-Hausman test for endogeneity. The test was conducted at the ten percent level.

higher: 0.238 for the value of production and 0.319 for cropland harvested. Thus, the national results are precise enough to rule out a substantial supply response, while the Heartland results, though failing to reject a null effect, are not as precise and do not rule out the possibility of an economically important effect.

Estimate Sensitivity to Instrument Endogeneity

For the IV model to provide unbiased estimates of the effect of payments for ZIP codes affected by the instrument, the past production of oilseeds must be exogenous to future changes in the value of production or cropland harvested. Holding key variables such as past production or area constant, it is unclear why farms in ZIP codes that previously produced more oilseeds would expand or intensify production more than farms in ZIP codes with less oilseed production, though the possibility cannot be ruled out. If the instrument is endogenous, the true parameter value will be given by (see supplementary appendix for the derivation):

$$(5) \quad \theta = \theta_{IV} - \frac{\text{cov}(\varepsilon_{07}, \text{Oilseeds}_{9702})}{\text{cov}(\text{Oilseeds}_{9702}, \Delta GP_{02-07})}$$

The term involving covariances is the bias term. In a sense, estimating $\text{cov}(\varepsilon_{07}, \text{Oilseeds}_{9702})$ requires estimating the direct effect of oilseed production on future expansion in production or cropland

harvested independent of the indirect effect through payments. We cannot separate the two effects in the study period, but we can look to a previous period (1992-2002) when oilseed production would have been largely unrelated to changes in program payments, and determine if it is statistically related to the outcomes in question. Formally, we estimate:

$$(6) \quad y_{i02} = \lambda_0 + \lambda_1 y_{i97} + \lambda_2 y_{i92} + \lambda_3 X_{i97} + \lambda_4 X_{i92} + \mu_{r(i)} + \eta_{i02}$$

and use the results to calculate the term $\text{cov}(\eta_{02}, \text{Oilseeds}_{9297})$. We then estimate the bias term in (5) by supposing that:

$$(7) \quad \text{cov}(\eta_{02}, \text{Oilseeds}_{9297}) \cong \text{cov}(\varepsilon_{07}, \text{Oilseeds}_{9702})$$

which we use to recover a “bias-corrected” θ . A potential problem with this approach is that the covariance between our instrument and the error term in a previous period (1992-2002) may not carry forward to the study period (1997-2007). Nonetheless, the exercise should provide insight into the magnitude of a possible bias.

In absolute terms, the bias terms are small, with the largest being 0.079 (table 5). Because the initial IV estimates were small, the bias term ranges from 14-169% of the original IV estimate. The magnitude and direction of the bias is the same for the national and Heartland

Table 5. Estimate of IV Bias

Sample	Outcome	Original IV Estimate	Bias Term Estimate	Bias as Percentage of Original Estimate	"Bias-Corrected" Estimate
Entire U.S.	<i>Value of Production</i>	−0.066	−0.012	19%	−0.053
	<i>Cropland Harvested</i>	−0.047	0.079	−169%	−0.126
Heartland	<i>Value of Production</i>	0.012	−0.020	−166%	0.032
	<i>Cropland Harvested</i>	0.139	0.035	25%	0.104

samples – it is small and negative for the value of production, and a bit larger and positive for cropland harvested. Incorporating the potential bias into the estimates therefore increases the estimated effect on the value of production and decreases the effect on cropland harvested. The exercise suggests that correlation between past oilseed production and the future value of production has little influence on IV estimates. For cropland harvested, the effect on estimates is slightly larger and implies that if anything, the IV estimates are biased upwards.

Further Robustness Checks

We perform further robustness checks to see if our results are sensitive to: 1) using oilseed production in 1992 and 1997 as the instrument (as opposed to 1997 and 2002); 2) adding further lags to account for serial correlation of the form AR(2); and 3) combining checks (1) and (2). The OLS and IV results for the coefficient on the change in payments are presented in table 6.

Oilseed production in 1992 and 1997 is arguably more exogenous to outcomes in 2007

than oilseed production in 1997 and 2002. Possible correlations between oilseed production and future outcomes are likely to weaken over time. Finding that average oilseed production for 1992 and 1997 is sufficiently correlated with changes in payments from 2002 to 2007, we re-estimate the main models using it as the instrument. Doing so has little effect on the point estimates of the effect of payments on the value of production or cropland harvested at the national level. In both cases, we reject the exogeneity of payments at the 10% level. When looking at the Heartland only, however, the point estimate increases and suggests a positive effect of payments on cropland harvested.

If cropping patterns and growth are driven by long-term dynamics, it may be important to control for longer lags in the dependent variable. As another robustness check, we add a set of lag variables corresponding to 1992, effectively making the model robust to serial correlation of the form AR(2). Before re-estimating the outcome equations, we check if the instrument is still relevant after adding a third lag, which it is (the lowest F-stat is 16.35). Adding the third lag leads to very small

Table 6. Summary of Robustness Checks

Robustness Check		Entire U.S.		Heartland	
		<i>Value of Production</i>	<i>Cropland Harvested</i>	<i>Value of Production</i>	<i>Cropland Harvested</i>
Using Oilseeds, 1992–1997 as instrument	OLS	0.206*** (0.014)	0.194*** (0.010)	0.293*** (0.021)	0.272*** (0.018)
	IV	−0.019 (0.061)	−0.051 (0.046)	0.136 (0.087)	0.202*** (0.071)
Controls for 1992 lagged dependent variable	OLS	0.196*** (0.013)	0.189*** (0.009)	0.287*** (0.021)	0.267*** (0.017)
	IV	0.016 (0.060)	0.018 (0.049)	−0.093 (0.128)	0.044 (0.115)
Combines the first two robustness checks	OLS	0.198*** (0.013)	0.190*** (0.009)	0.290*** (0.021)	0.270*** (0.018)
	IV	0.009 (0.055)	−0.025 (0.040)	−0.093 (0.101)	0.088 (0.075)

Note: Asterisks (***, **, *) denote significance at the 1%, 5%, and 10% levels, respectively. Robust standard errors are in parenthesis.

point estimates for the effect of payments (less than 0.020) at the national level. For the Heartland, adding a third lagged dependent variable pushes the point estimate for the effect on the value of production to negative (-0.093), while decreasing the estimated effect of payments on cropland harvested to 0.044, which is statistically indistinguishable from zero.

Combining the two robustness checks points to a small, if present at all, effect of payments on production or area harvested. At the national level, the point estimate for the effect of payments on production and area is again less than 0.020. Furthermore, the statistically significant effect for cropland harvested in the Heartland found when using oilseed production from 1992 and 1997 disappears, with the point estimate decreasing from 0.202 to 0.088.

Discussion

Using a sample of ZIP codes that account for more than 80% of the total U.S. production of program crops (as we define them), we conclude that there is little evidence that decoupled payments affect production. ZIP codes where farms on the whole received a greater increase in payments from the 2002 Farm Act due to greater historic oilseed production did not see larger increases in the value of program crops production compared to ZIP codes where farms had less favorable changes in payments. The same applies to the relationship between payments and cropland harvested.

Focusing on the Heartland, the results are less conclusive, though they generally concur with the findings from the national analysis. Using oilseed production in 1992 and 1997 as an instrument instead of oilseeds production in 1997 and 2002 shows an economically large effect of payments on production and cropland harvested, but the finding is not robust when a third set of lagged variables is added. Rotation patterns are perhaps stronger in the more homogenous heartland, thus increasing the importance of controlling for longer lags. It is also possible that the growth in ethanol plants, which was concentrated in the Heartland, increased prices in local markets and encouraged farmers to use payments to finance expansion.

In all cases, OLS provides large and precisely estimated positive effects of payments on production and cropland harvested, which is unsurprising given the mechanical correlation between growth in production and payments.

In most cases, controlling for the endogeneity of payments produced coefficient estimates that were significantly smaller than the OLS estimates and also statistically different from them. While OLS estimates have smaller variances than IV, the efficiency losses are limited by the strength of the relationship between our instrument (past oilseed production) and the endogenous variable (changes in payments). The lack of a clear effect of payments on production or cropland harvested, therefore, cannot be readily attributed to a weak instrument.

On the upper end, the confidence intervals from our main IV estimates reject an elasticity between payments and production and area harvested greater than 0.072 and 0.067. Gardner et al. (2010) estimated that a 50% decrease in commodity payments would reduce cropland in the United States by 22%, implying an elasticity of 0.44; their estimate is about double our OLS estimate for the national sample, and our IV estimates suggest that OLS is biased upwards.¹⁸ There are many possible explanations for the difference, but perhaps the most likely reason is that commodity payments were explicitly linked to production in two of the three years covered by the Gardner et al. study (1987 and 1992).

Because the empirical model from O'Donoghue and Whitaker (2010) is based on changes to the average behavior of a state-commodity cohort of farms, it is roughly comparable to an aggregate analysis. These authors find that the 2002 policy change increased payments to the average farm by about 40%. The associated increase in acreage ranged from 9-16% and implies an elasticity between payments and acreage in the range of 0.23 and 0.40. These estimates are larger than our OLS estimates and well beyond the range of values suggested by the 95% confidence intervals of the IV estimates. Statistical issues associated with using ARMS data in a pseudo-panel analysis might explain these discrepancies.

Conclusion

Employing an identification approach that relies on the provision of the 2002 Farm

¹⁸ We also aggregate farms to the county level (the level of the Gardner et al. study) and estimate our main model. The point estimates for both the OLS and IV models are all less than 0.06 and both of the IV point estimates are slightly negative. The full results can be found in the supplementary appendix.

Act, which made oilseeds eligible for decoupled payments, we estimate the total supply response to changes in decoupled payments for ZIP codes that account for more than 80% of the total value of program crop production. Our findings suggest that from 2002-2007, decoupled government payments had little effect on the value of program crop production. The results do not imply that decoupled government payments will always have such neutral effects on production. How farmers use the extra income from payments depends on numerous factors, including market conditions.

While it is reasonable to expect government payments to affect program crop production, our analysis also allows payments to affect the production of non-program crops. We do this by examining the effect of payments on total cropland harvested, which includes program and non-program crops. The findings for cropland harvested are generally consistent with those from looking at the production of program crops. We find some evidence that payments increased cropland harvested when the analysis is restricted to the Heartland, but the result does not hold up when controlling for longer time trends.

Decoupled payments are sometimes justified by claiming that they help to secure an abundant and stable food supply, but our results suggest that such claims are overstated. At the same time, the results do not support the critique that decoupled payments cause excess production and therefore distort world commodity prices and trade. Under current budget constraints, however, the most likely policy scenario is a decrease in decoupled payments. Countries that are major producers of agricultural commodities would likely welcome a reduction in U.S. domestic support, especially countries like Canada and Brazil, which in the past have lodged formal WTO complaints over U.S. agricultural subsidies. Our findings imply that the reduction or removal of decoupled commodity payments would have modest effects on U.S. agricultural production, and by extension on world markets.

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